# SCIENCE

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

ILLUSTRATED.

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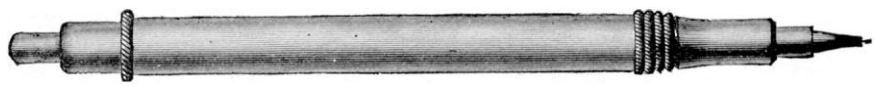

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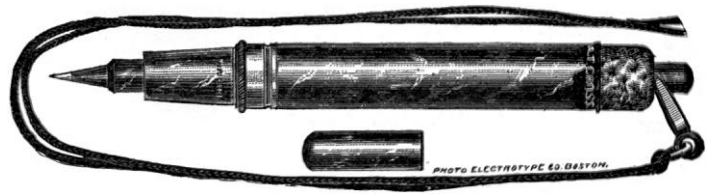


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All communications should be addressed to the Editor—box 3838, P. O., New York—with name and address of writer, not necessarily for publication without consent.

Scientific papers and correspondence intended for publication should be written *legibly* on one side only of the paper. Articles thus received will be returned when found unsuitable for the Journal.

Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply may be written in the form of an article.

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JOHN MICHELS, Editor.

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229 BROADWAY, NEW YORK.

P. O. Box 3838.

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Dr. Beard described, briefly, the various forms of trance with which neurologists are familiar, and was supported by eight trance subjects, who exhibited manifestations of trance phenomena, to the equal satisfaction of the lecturer and his appreciative audience.

In regard to the genuineness of Dr. Beard's demonstrations we have no doubt that, substantially, they were *bona fide*, but it seemed apparent that the miserable objects who did duty on the occasion overacted their parts, and it may be even now an open question whether Dr. Beard or his audience was more imposed upon. Without intending to assert that an imposition was intended or practiced on the occasion, it is not difficult to show, probably, that many of the experiments might have been illusions. Two of the so-called patients were evidently trained performers, if not professional actors; if merely amateurs they surely missed their vocation. One of these patients could throw himself from an erect position to the stage, on his face, with the ease of an acrobat; the other declaimed Shakespeare at short notice, with the energy and persistence of a barrel organ. Other experiments also developed phenomena, which were not part of the programme. The boy who ate Cayenne pepper in a trance, believing it to be sugar, appeared to be not inconvenienced in the least when he returned to a normal condition. But still more remarkable was the behavior of the patient who was made "stone deaf." Dr. Beard shouted in vain to this man, a tuning fork was sounded, a bell rung, and even a pistol fired close to his devoted head, while the patient remained eloquently silent and apparently oblivious to all external sounds.

To de-hypnotize the subject, Dr. Beard, unmindful of the fact that he was supposed to be addressing a deaf person, said, in an ordinary tone of voice: "It's all right!" that being the usual phrase employed. To the surprise of many present, the patient (perhaps not desiring a *contretemps* to mar the performance) took the cue and quietly resumed his seat.

To a popular audience Dr. Beard's theories and experiments might have partaken of the character of a revelation, but we believe that nearly all our present knowledge of the subject dates from Braid's book on Hypnotism, published more than twenty years ago.

The policy of such public exhibitions may be well questioned; in Vienna they have been prohibited, and as no new truth can be gained or science advanced by repeating these experiments in such a manner, why make them the subject for an evening's amusement before a scientific society?

The patients selected perform their parts constantly, and thus become finally, perhaps unconsciously, more and more trained to elaborate their antics, so that, even admitting the genuineness of the performance, the experiments may be, at least so far, manufactured.

The subjects of Dr. Beard are chiefly selected from the nervous classes of our population, and although they may be willing to air their peculiarities before a fashionable audience, it would appear to be a charitable course to keep them from such public exhibitions which can result only in aggravating their morbid tendencies.

## NEW YORK ACADEMY OF SCIENCES.

The Committee on Lectures announces that the remainder of the course will embrace five lectures, to be delivered at the new Hall of the Academy of Medicine, No. 12 West Thirty-first street, New York City, on the third Monday of each month.

These lectures are free to the public, but admission is strictly confined to those holding tickets, which may be obtained of D. S. Martin, 236 West Fourth street; W. P. Trowbridge, School of Mines, East Forty-ninth street, and Alex. A. Julien, School of Mines, East Forty-ninth street.

The programme includes the following lectures: January 17th, Respiration, by Dr. J. W. S. Arnold; February 21st, The Reptilian Affinities of Birds, by Professor Edward S. Morse; March 21, Sensation and Pain, by Dr. Charles Fayette Taylor; April 18th, Temple Architecture of the Tenth to the Fifteenth Century, by Professor George W. Plympton; May 10th, The Organic Elements, by Professor Albert R. Leeds.

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## HUNGER THE PRIMITIVE DESIRE.

By S. V. Clevenger, M. D.

A paper on Researches into the Life History of the Monads by W. H. Dallinger, F. R. M. S., and J. Drysdale, M. D., was read before the Royal Micrscopical Society, Dec. 3d, 1873, wherein fission of the Monad was described as being preceded by the absorption of one form by another. One Monad would fix on the sarcode of another and the substance of the lesser or under one would pass into the upper one. In about two hours the merest trace of the lower one was left and in four hours fission and multiplication of the larger monad began. A full description of this interesting phenomenon may be found in the *Monthly Microscopical Journal* (London), for October, 1877.

Professor Leidy has asserted that the Amœba is a cannibal, whereupon Mr. J. Michels in the *American Journal of Microscopy*, July, 1877, calls attention to Dallinger and Drysdale's contribution and draws therefrom the inference that each cannibalistic act of the Amœba is a reproductive one, or copulative, if the term is admissible, The editor (Dr. Henry Lawson), of the English journal, Oct., 1877, agrees with Michels.

Among the numerous speculations upon the origin of the sexual appetite, such as Maudsley's altruistic conclusion, which always seemed to me to be far-fetched, I have encountered none that referred its derivation to *hunger*. At first glance such a suggestion seems ludicrous enough, but a little consideration will show that in thus fusing two desires we have still to get at the meaning and derivation of the primary one—desire for food.

The cannibalistic Amœba may, as Dallinger's Monad certainly does, impregnate itself by eating its own kind, and we have innumerable instances among Algæ and Protozoa of this sexual fusion appearing very much like ingestion. Crabs have been seen to confuse the two desires by actually eating portions of each other while copulating, and in a recent number of the *Scientific American*, a Texan details the *Mantis religiosa* female eating off the head of the male Mantis during conjugation. Some of the female Arachnidæ find it necessary to finish the marital repast by devouring the male, who tries to scamper away from his fate. The bitings and even the embrace of the higher animals appears to have reference to this derivation. It is a physiological fact that association often transfers an instinct in an apparently outrageous manner. With quadrupeds it is undoubtedly olfaction that is most closely related to sexual desire and its reflexes, but not so in man. Ferrier diligently searches the region of the temporal lobe near its connection with the olfactory nerve for the seat of sexuality, but with the diminished importance of the smelling sense in man the faculty of sight has grown to vicarate olfaction; certainly the "lust of the eyes" is greater than that of other special sense organs among Bimana.

In all animal life multiplication proceeds from growth, and until a certain stage of growth, puberty, is reached, reproduction does not occur. The complementary nature of growth and reproduction is observable in the large size attained by some animals after castration. Could we stop the division of an Amœba a comparable increase in size would be effected. The grotesqueness of these views is due to their novelty, not to their being unjustifiable.

While it would thus seem apparent that a primeval origin for both ingestive and sexual desire existed, and that each is a true hunger, the one being repressible and in higher animal life being subjected to more control than the other, the question then presents itself: What is hunger? It requires but little reflection to convince one of its potency in determining the destinies of nations and individuals, and what a stimulus it is in animated creation. It seems likely that it has its origin in the atomic affinities of inanimate nature, a view monistic enough to please Haeckel and Tyndall.

## NOTES ON THE ANATOMY OF THE ENCEPHALON, NOTABLY OF THE GREAT GANGLIA.

By Edward C. Spitzka, M. D.

The anatomy of no portion of the brain is so obscure and so imperfectly known as that of the so-called Thalamus opticus. One of the first requisites to a comprehension of its relations is the establishment of a proper nomenclature, and the point to start from is the very name under which the great ganglionic mass is known. Since it is not exclusively or even in the main connected with the optic tracts in any animal or man, and, indeed, is in the lower sauropsidæ and amphibians not connected with them at all, the affix *opticus* should be dropped, and the first word involving that very uncompromising conception of an elevation at the ventricular floor may be retained: *Thalamus*.

The current conception that the Thalamus is an elevation at the floor of the *lateral* ventricle is incorrect. One of our leading comparative anatomists will shortly review this question, and it will therefore be but necessary for me to refer to the matter.

In the cat's brain it can be clearly seen, that (aside from membranous separations) the great mass of the Thalamus is excluded from the cavity of the lateral ventricle by the fusion of the lateral edge of the fornix with the corpus striatum, or rather with the ependyma of that ganglion. Consequently, the two thalami are included in the third ventricle, which cavity on cross section resembles an upright T, whose vertical branch descends between the thalami, as a deep ditch, the *vulva cerebri* of the old anatomists.[1]

Luys, who was unfortunately wedded to certain physiological prejudices as to the function of the thalamic centres, restricted the term *Thalamus* to the most external mass. Meynert called all the centres in the aggregate by that term as a collective designation. He excluded, however, that gray mass which lines the sides of the vertical slit of the third ventricle.

Now, the third ventricle, as shown by Hadlich and Wilder, extends over the entire thalami; it would be, therefore, incorrect to limit the designation "central tubular gray of the third ventricle" to that portion only which lines the vertical slit. Either this latter designation should be extended to the entire thalamic masses or the term thalamus should be extended to the so-called central tubular gray.

Thus interpreted there would be, strictly speaking, but a single thalamus, consisting of two main masses, and a commissural part. The commissure is double. The thalami are primitively united by the lower of these commissures, which I propose to term "basilar commissure."[2] Secondarily, and only in animals above marsupials (as far as I am aware), do we find another commissure produced at an advanced period of embryonic development by apposition of the main masses. This is the so-called middle commissure of the brain, the *commissura grisea, c. mollis*. I should consider the least ambiguous designation, "the *thalamic fusion*."

In a manner similar to that which separates the candate and lenticular nuclei from each other, and which divides the latter into subsidiary "articuli," the chief mass of each thalamus is separated into an inner and outer zone. The zones are separated from each other by

[1] The corresponding *penis cerebri* of the same anatomists has, by more fastidious colleagues, been rebaptized *pinus cerebri* and later *pineal gland*, now known as the *epiphysis cerebri*.

[2] Continuous in front with the *loci perforati antici*, behind with the infundibulum. Atrophic over the chiasm, it exhibits a set of transverse fibres and gray substance elsewhere.

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In a manner similar to that which separates the candate and lenticular nuclei from each other, and which divides the latter into subsidiary "articuli," the chief mass of each thalamus is separated into an inner and outer zone. The zones are separated from each other by

[1] The corresponding *penis cerebri* of the same anatomists has, by more fastidious colleagues, been rebaptized *pinus cerebri* and later *pineal gland*, now known as the *epiphysis cerebri*.

[2] Continuous in front with the *loci perforati antici*, behind with the infundibulum. Atrophic over the chiasm, it exhibits a set of transverse fibres and gray substance elsewhere.

a white intercalation, and especially the outer zone (also in part the inner) presents a beautiful alternation of gray and white laminæ.[3]

These two gray zones constitute the fundamental demarcation of the thalamus; they may be termed *zona grisea medialis* and *zona grisea lateralis*. In animals above the rank of marsupials we find added a round nodular mass, distinctly prominent at the ventricular floor, which lies anteriorly, while in still higher groups a second nodular prominence develops posteriorly. The latter is known as the posterior tubercle or pulvinarium, the former as the *anterior* or *superior tubercle*. The former designation seems the best to me, for although what I call the undifferentiated parent mass of the thalami is visible in sections anterior to those in which the anterior tubercle is reached, yet the latter, which I propose to term the *anterior nodule* of the thalamus, is the first differentiated centre reached. In man the *zona grisea medialis* is faintly seen before the anterior nodule is reached, but the anterior nodule reaches its main development before the zones do, and is absent where these are most prominent. In the carnivora generally, the anterior nodule projects far in advance of the zones. In these animals, too, a more complex arrangement of this nodule is found than in man, inasmuch as the anterior part of the internal slope of the thalamus shows several elevations absent in the human thalamus.

The *zona grisea medialis* appears pretty equally diffused and exhibits its lamination evenly both in front and in the middle of its course. The same applies to the human brain for the *zona grisea lateralis*. In the cat,[4] however, the anterior part of the external zone appears as a beautiful round compact ganglionic mass, protruding boldly into the internal capsule, and which acquires the characteristic lamination only in posterior planes.

It is interesting to note that the ganglionic matter of the thalamus is continuous with that of the ventricular nucleus of the corpus striatum (nucleus caudatus). Indirectly it is connected with the extra-ventricular nucleus, through that great common basilar gray mass, which is the *rendezvous*, as it were, of all the gray categories of the forebrain.[5]

In an earlier publication (Architecture and Mechanism of the Brain—Journal of Mental and Nervous Diseases, 1879), I have called attention to the fact that the ventricular nucleus of the corpus striatum is the representative of the primordial cerebral gray, inasmuch as the nerve cells of the embryonic and lower amphibian hemisphere are concentrated immediately subjacent to the ependyma of the latter ventricle. The majority of these cells are crowded away from the ventricular floor by the white substance developed in higher animals, and only a portion of the primitive gray remains subependymal. This is precisely what constitutes the corpus striatum. Now the corpus striatum actually *lines* the ventricle; it not only lies at its floor! Any section transversely to the cerebral axis and striking the forepart of the lateral ventricle in the Hippopotamus, Horse, Dog or Cat, will show that an attenuated part of the corpus striatum is continued around *over* the ventricle, and constitutes a greater part of its roof.

A similar comparative study shows that the nucleus lenticularis is also a subcortical development, that is, it results from the individualization of a gray mass originally continuous with the cortex, by means of an irruption of white masses. These at first separate fasciculi (as in the dog) in higher animals coalesce to constitute the external capsule. The segmentation of the lenticular nucleus into three distinct *articuli* so characteristic of the human brain, is not found in the carnivora; only the outer articulus is demarcated, and that but imperfectly.

In the carnivora the *laminae medullares* or white streaks of the lenticular nucleus are conspicuously absent in the anterior half of that ganglion; in its posterior half they appear and they rapidly increase in bulk as we proceed backwards, so that in planes where the human lenticular nucleus is still quite massive, we have in the dog only slight ganglionic masses intercalated between the fibre tracts. The *claustrum* is, in the carnivora, not the thin expanded lamina found in man, but a low and massive accumulation, hardly separated from the cortex of the Island of Reil. This fact strengthens Meynert's view that the claustrum is but an individualized cortical layer.

In conclusion I would mention as an isolated fact, and disconnected from the main subjects dealt with in these notes, that the anterior pyramids of the brain of the large Ceylon fruit bat (Pteropus fuliginosus) undergo a superficial decussation, as patent, and more so, as that of the optic chiasm. The pyramidal tract after decussating is continued as a distinct fasciculus on the lateral aspect of the medulla oblongata. In the same brain the fibres of the fornix can be clearly seen to terminate in the thalamus without descending to the base of the brain. Whether this applies to the whole of that tract, I am not able to say.

I would also note that in the brain of a large Ara (*Ara ararauna*) obtained from the Superintendent of the Central Park Zoological Gardens, Mr. W. A. Conklin, I found what appeared to be a thin commissure uniting the two cerebral hemispheres in their posterior half. This (commissure! if the observation was correct) was not, like the Corpus callosum, a connection between the internal white matter of both hemispheres, but merely a union of the superficial white, which in lower animals is well-developed outside of the cortical gray.

In the *carnivora the Ganglion* of Soemmering (the *Substantia nigra* in the human brain) is continuous with the innermost part of the lenticular nucleus. This fact strengthens Meynert's proposition, that the Ganglion of Soemmering, like the caudate and lenticular nuclei, should be considered as parts of one system, whose ganglia are connected with the fibres of the *pes pedunculi*.

In the elephant, whose brain, both in its mass, the preponderance of the hemispheres, and the concealment from view of the so-called "trapezium," takes a high rank as regards the grade of development, I had the opportunity to make and examine transverse microscopic sections from the Pons Varolii. The remarkable discovery was made that the descending (longitudinal) fibres of the Pons are wanting. Nothing but transverse fasciculi are seen in the field. Since the former fibres constitute part of the pyramidal tract, it follows that the tract of the voluntary impulses, the "will-tract," must take another course in the elephant, one which may be considered aberrant; for in all other placental animals so far examined by myself, the pyramidal tract runs through the Pons Varolii, as in man.

[3] And yet the latest pretended description of these Ganglia, admitted, notwithstanding numberless glaring errors, into a journal of the standing of "Brain" (that by Dalton), has the Thalamus "homogenous."

[4] As seen in a series of transverse sections prepared by Dr. Graeme Hammond.

[5] Here meet the olfactory gray, the cortex, the *basis capitis nuclei caudati*, the *nucleus lenticularis*, the *claustrum*, the thalamic axial gray, etc., etc.

ON WALDIVINE.—Waldivine, $C_{36}H_{24}O_{20}$, is a neutral principle, without rotatory power, very sparingly soluble in cold water, freely soluble in chloroform, insoluble in ether, and remarkable for the ease with which it is decomposed by alkalies.

CERTAIN OPTICAL AND VISUAL PHENOMENA—If the flame of a lamp is viewed through a narrow slit, the lustre of the flame and the effects of diffraction vary much according as the slit is vertical or horizontal, the light being much more considerable in the latter case.—M. TREVE.

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As far as I have seen, I have never observed contraction of muscles, areas of hyperæsthesia, or other disorder of sensibility, or any unnatural condition or action of any part of the body in the persons affected, unless the operator should direct their attention to themselves by speaking or motioning to them; for example, he would indicate that their faces were away, that their arms or fingers were stiff, or that they had a pain in the head, back, or some other part. In such a case what was told them would be the basis on which they would feel or act.

If I should venture an explanation, or more properly a description of the phenomena of hypnotism, I would say that they resulted from a suspension of function of the centre for ideas in the brain of the subject, and also of his will, while the infra-cortical ganglia remain free to act from a reflex excitation imparted by the voice, gestures, or manners of the operator.

## THE HAMMOND PRIZE.

The American Neurological Association offers a prize of five hundred dollars, to be known as the "William A. Hammond Prize," and to be awarded, at the meeting in June, 1882, to the author of the best essay on the *Functions of the Thalamus in Man.* The conditions under which the prize is to be awarded are as follows: 1. The prize is open to competitors of all nationalities. 2. The essays are to be based upon original observations and experiments on man and the lower animals. 3. The competing essays must be written in the English, French, or German language; if in the last, the manuscript is to be in the Italian handwriting. 4. Essays are to be sent (postage prepaid) to the Secretary of the Prize Committee, Dr. E. C. Seguin, No. 41 West Twentieth street, New York City, on or before February 1, 1882; each essay to be marked by a distinctive device or motto, and accompanied by a sealed envelope bearing the same device or motto, and containing the author's visiting card. 5. The

successful essay will be the property of the Association, which will assume the care of its publication. 6. Any intimation tending to reveal the authorship of any of the essays submitted, whether directly or indirectly conveyed to the Committee or to any member thereof, shall exclude the essay from competition. 7. The award of the prize will be announced by the undersigned Committee; and will be publicly declared by the President of the Association at the meeting in June, 1882. 8. The amount of the prize will be given to the successful competitor in gold coin of the United States, or, if he prefer it, in the shape of a gold medal bearing a suitable device and inscription.

Signed, { F. T. MILES, M.D., *Baltimore.*
J. S. JEWELL, M.D., *Chicago.*
E. C. SEGUIN, M.D., *New York.*

## CHESAPEAKE ZOOLOGICAL LABORATORY.

Dr. W. K. Brooks, Director of the Chesapeake Zoological Laboratory, established under the auspices of the the Johns Hopkins University, in his report for 1880 states: By the liberality of the Trustees, it was possible to spend a much longer period than hitherto at the seaside, and provided with a more liberal outfit, including a steam launch which was built, for our use in the last spring, at Bristol, R. I., and has proved a very efficient auxiliary. The necessary books, dredges, and other instruments were also provided by the University. In addition to the opportunities afforded to three of the members of our own academic staff, three other gentlemen, devoted to the study of Zoology, were invited to avail themselves of the scientific facilities of the station.

The laboratory was opened at Beaufort, N. C., on April 23, 1880, and closed on September 30, after a session of twenty-three weeks. It was supplied with working accommodations for six investigators, and the facilities which it afforded were used by the following six persons: W. K. BROOKS, PH. D., Director; K. MITSUKURI, PH. B., Fellow in Biology; E. B. WILSON, PH. B., Fellow in Biology; F. W. KING, A. M., Professor of Natural Science, Wisconsin State Normal School; H. C. EVARTS, M. D., Academy of Natural Sciences, Philadelphia; H. F. OSBORNE, PH. D. Fellow of the College of New Jersey.

Beaufort was selected for our third season's work because it is the nearest accessible town, south of Baltimore, which is favorably situated for zoological study. The advantages of a location in a town are well shown by the fact that the expenses of a session of twenty-three weeks this year were considerably less than those of a ten weeks session the year before.

The scientific advantages of Beaufort are very great; the most important is the great difference between its fauna and that of our northern Atlantic coast.

The configuration of our coast line is such that Cape Hatteras, the most projecting point south of New York, deflects the warm water of the Gulf Stream away from the coast, and thus forms an abrupt barrier between a cold northern coast and a warm southern one. The fauna north of this barrier passes gradually into that of southern New England, while the fauna south of this barrier passes without any abrupt change into that of Florida, but the northern fauna is sharply separated by Cape Hatteras from the southern.

As the laboratory of the U. S. Fish Commission and Mr. Agassiz's laboratory at Newport afford opportunites for work upon the northern fauna, it seemed best for us to select a point south of Cape Hatteras in order to study the southern fauna with the same advantages, and as Beaufort is the only town near the Cape which can be reached without difficulty, it was chosen as the best place for the laboratory.

The situation of this town is exceptionally favorable for zoölogical work, for the surrounding waters present such a diversity of conditions that the fauna is unusually rich and varied.

Close to the town there are large sand bars, bare for miles at low tide, and abounding in animal life. From these we could collect an unfailing supply of *Amphioxus, Renilla; Limulus, Balanoglosus*, Sea Urchins, and a great variety of Molluscs and Crustacea.

The mud flats furnished us with another fauna, and yielded a great variety of Annelids, a new set of species of Crustacea and Molluscs, Gephyreans, Echinoderms, and Polyps. The large salt marshes gave us a third fauna, and a short distance inland large swamps of brackish and fresh water furnished still other conditions of life.

As the town is situated at the point where Gore Sound connects Pamlico Sound with Bogue Sound we were within easy reach of a continuous sheet of landlocked salt water more than a hundred miles long, and these Sounds furnished still another collecting and dredging ground, abounding in Corals, Gorgonias, Ascidians, Star Fish, Sea Urchins, and a new set of Molluscs and Crustacea.

As most of the shores are flat and sandy, those animals which live upon a sandy bottom are much more abundant than those which attach themselves to solid bodies, but the stone breakwaters at Fort Macon, the wharves at Beaufort and Morehead City, and the large oyster beds which are found in the sounds furnish a proper habitat for many fixed animals, and yielded us a rich supply of Hydroids, Corals, Ascidians, Sea Anemones, Sponges, Cirrhipeds, &c. The ocean beach, within a short distance of the town, furnished still another fauna, and a soil of three miles from the laboratory carried us to a good locality for ocean dredging.

The greatest advantage of the locality is the richness of its pelagic fauna. There are very few points upon land which are so situated that the surface animals of mid-ocean can be procured in abundance for laboratory work, and as careful work is very difficult on shipboard, a laboratory which can be furnished with a good supply of living pelagic animals presents opportunities for work in an extremely interesting and almost new field.

The Gulf Stream is constantly sweeping these animals northwards along the North Carolina coast, and as the tide sets in through Beaufort Inlet into the Sounds the floating animals are carried with it. Such oceanic animals as *Physalia* and *Porpita* were frequently thrown, uninjured and in perfect health, upon the beach within twenty feet of the laboratory, and during the season we found nearly all the Siphonophoræ which are known to occur upon our Atlantic coast.

With all these advantages we enjoyed a mild and uniform climate which enabled us to work in perfect comfort during the hottest months of summer.

The zoölogical resources of Beaufort have not escaped the attention of American naturalists, and there are few places upon our coast, outside of New England, where more zoölogical work has been done. In 1860, Drs. Stimpson and Gill spent a season in dredging and collecting in the vicinity of Beaufort, Cape Lookout and Cape Hatteras, and an account of their work was published in *The American Journal of Science.* Dr. Coues, who was stationed at Fort Macon during the war, occupied himself for two years in collecting the animals which are found here, and he published a series of papers on the "Natural Histsry of Fort Macon and Vicinity" in the Proceedings of the Academy of Natural Sciences of Philadelphia.

These papers, which were continued by Dr. Yarrow, contain copious and valuable notes on the habits and distribution of the animals which were observed, and we found them a great help to us. These two naturalists found four hundred and eighty species of animals in the vicinity of Beaufort. Of these four hundred and eighty, two hundred and ninety-eight are vertebrates, and one

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hundred and eighty-two are invertebrates. Of the vertebrates twenty-four are mammals, one hundred and thirty-three are birds, twenty-seven are reptiles, six batrachians, ninety-seven fishes and eleven selachians. Of the invertebrates, one hundred and forty-seven are molluscs, twenty-one are crustaceans. The list of vertebrates is very nearly exhaustive, and we made no additions to it, but the list of invertebrates is obviously very imperfect, and, although we made no attempt to tabulate the species which we observed, there would be no difficulty in enlarging the list twenty or thirty fold.

Among other naturalists who have spent more or less time at Beaufort, I may mention Professor L. Agassiz, Professor E. S. Morse, Dr. A. S. Packard, Professor Webster, and Professor D. S. Jordan. Professor Morse procured most of the material for his well known paper on the Systematic Position of the Brachiopoda on the sand bars in Beaufort Inlet.

I will now attempt to give a very short statement of some of the leading points in our own summer's work. Much of our time was spent in studying the development of the Crustacea, since this is one of the most important fields for original work upon our southern coast. The supply of material is almost inexhaustible, and would employ a number of students for many years. The life history of the Crustacea is of great interest in itself, and the recent species are so numerous and diversified that there is no group of animals better adapted for studying the general laws of embryonic development in their relation to the evolution of the group.

These considerations have led us to devote especial attention to this group during this and the preceding seasons. One of the published results of the first season's work was an illustrated account of the metamorphosis of *Squilla*, a representative of a somewhat aberrant group of Crustacea. During the second season, a member of our party, Professor Birge, made a very thorough study of the development of *Panaſæus*, one of our crabs, and the account of his observations, with drawings, was ready for publication several months ago. At Beaufort, we spent most of our time upon this subject, and figured more than eight hundred points in the development of various Crustacea.

Among these, I wish to call especial attention to our observations upon the development of the Sergestidae; the least specialized of the stalk-eyed Crustacea. This very peculiar group was not known to occur upon our coast until we found a few specimens of one genus at Fort Wool, and the same genus —*Lucifer*—in great abundance at Beaufort, associated with another genus which is also new to North America. As nothing whatever was known of the development of *Lucifer*, we made every effort to obtain the eggs and young, and after four months of almost fruitless labor we finally succeeded in finding all the stages of the metamorphosis, and figured them in a complete series of ninety-nine drawings. We also obtained a somewhat less complete series of figures of stages in the life history of the second Sergestid.

Our only motive in this work was the desire to fill a gap in our knowledge of crustacean development, by supplying the life history of a very interesting group of animals, but the result was found to have a very unexpected value, since it contributes to the discussion of a number of problems in general embryology and morphology, and is the most significant crustacean life history which has ever been studied.

The following are some of the more important points:

The egg undergoes total regular segmentation.

There is no food yolk, and cleavage goes quite through the egg.

There is a true segmentation cavity.

Segmentation is rhythmical.

There is an invaginate gastrula.

The larva leaves the egg as a Nauplius, and passes through a protozoan stage, and a schizopod stage.

The fifth thoracic segments and appendages are entirely wanting at all stages of development.

Another interesting group which was studied is the Porcellanidae, the least specialized of the true crabs. The adults of our American species are almost restricted to our southern waters, although the swimming larvæ are carried north by the Gulf Stream. Within the last two years two northern naturalists have studied these floating embryos upon the south coast of New England, but as they were working upon stragglers so far from home their accounts are incomplete and somewhat contradictory. Our advantages at Beaufort enabled us to contribute towards the solution of this confused subject by raising one species of *Porcellana* from the egg.

We also raised six other species of crabs from the egg, and made drawings of the more important stages of development. One of the species which was thus studied is the edible crab. Its metamorphosis has never been figured, and although it presents no unusual features, its economic importance gives value to exact knowledge of its life history.

Mr. Wilson also studied the development of one species of Pycnogonida, a group of very peculiar Arthropods, distantly related to the spiders. As he has paid especial attention to the systematic study of this group, and is now engaged in describing the Pycnogonids collected in the Gulf Stream by Mr. Agassiz, the opportunity to study them alive in the laboratory has been a great advantage to him.

Another important investigation is the study, by Mr. Wilson, of the embryology of the marine Annelids. Although the representatives of this large group are abundant and widely distributed, little was known of the early stages of their development until he procured the eggs of several species and studied them at Beaufort. This investigation has shown, among other things, that the accepted division of Annelids into two great groups, the Oligochæta and Polychæta, is not a natural method of classification. The work upon the development of marine Annelids was supplementary to an investigation which Mr. Wilson carried on last spring at Baltimore, and which he will continue this winter, upon the development of land and fresh water Annelids.

As much time as possible was given this season to the study of the hydroids and jelly-fish of Beaufort. The life histories of several of them were investigated, a thorough anatomical study of some of the most important forms was carried on, and nearly two hundred drawings were made. It is almost impossible to complete a study of this kind in a single season, but if one or two more summers can be given to the work, we have every reason to hope for valuable results; for although the North Carolina coast is the home of many species which are only found as stragglers upon our northern coast, and of other species which are not known to occur anywhere else, and of some genera and families which are new to the North American coast, this field has suffered almost total neglect.

Nearly three months of the time of two members of our party, Mitsukuri and Wilson, were given to the study of the habits, anatomy and development of *Renilla*, a compound Polyp very much like that which forms the precious coral, but soft and without a stony skeleton. The animals which form the community are so intimately bound together that the community, as a whole, has a well marked individuality distinct from that of the separate animals which compose it. The compound individuality of *Renilla* is quite rudimentary as compared with that of a Siphonophore, and as there is no trace of it in the closely allied Gorgonias, it furnishes an excellent field for studying the incipient stages in the formation of a compound organism by the union and specialization of a community of independent simple organisms. With this end in view the anatomy of the fully developed community was care-

fully studied, and the formation of a community was traced by rearing a simple solitary embryo in an aquarium until a perfect community has been developed from it by budding. During the process of development the law of growth by which the characteristics of the compound organism are brought about was clearly exhibited, and it is fully illustrated by nearly one hundred drawings.

One of the most interesting results of our work is the explanation, by Mr. Wilson, of the origin of the metamorphosis of the larva of *Phoronis*, a small Gephyrean worm which lives in a tube. Several of the most noted embryologists of Europe have studied the development of *Phoronis*, and our knowledge of its life history is due to their combined labors. Last summer Mr. Wilson reviewed the subject, and added some important points, and during the present season he has shown by the comparison of a great number of allied forms, that the very peculiar metamorphosis admits of an extremely simple explanation. The adult is sedentary and confined to its sand tube, while the larva is a swimming animal totally different in structure. The change from the larva to the adult is very rapid and violent. It occupies only a few minutes, and during the change the larva becomes turned wrong side out, so that what was internal is external. Mr. Wilson's comparison shows that *Phrronis* was originally a free animal, and that the structural peculiarities which fit the adult for sedentary life in a tube are of recent acquisition. The larva has, however, retained its ancestral adaptation to a swimming life in order to provide for the distribution of the species. There must have been a time, in the evolution of the species, when the adult was imperfectly adapted to a sedentary life, and also imperfectly adapted to a swimming life, and if the development of the individual were a perfect recapitulation of all the stages in the evolution of the species, we should have, between the swimming larva and the sedentary adult, a stage of development during which the adaptation is not quite perfect for either mode of life. It is clearly an advantage for the animal to pass through this stage as quickly as possible, or to escape it altogether. The peculiar metamorphosis enables the larva to remain perfectly adapted to a locomotor life until the occurrence of the sudden change which fits it for life in a tube, and Mr. Wilson has pointed out the manner in which the metamorphosis has been acquired in order to bridge over the period of imperfect specialization. This explanation is somewhat similar to that which Lubbock has given of the origin of the metamorphosis of insects, and we may hope that the same method of investigation will throw light upon the significance of other remarkable instances of metamorphosis in the Invertebrates.

## THE MATERIALISTIC ORIGIN OF THE SEXES.

By Andrew Dewar.

Materialism is yet in its infancy. Born of human learning, weaned in scientific research, and cradled in the toleration of an enlightened civilization, its advent marks an epoch in the history of humanity. Should there be fearful shadows in its progress, where loiter grim doubts and gloomy forebodings, these are only consequent to its youth, and the necessary result of the light from a sun whose slanting rays only reach us. But even as the noonday sun chases away the shadows in its splendor, so we are assured that no doctrine in these enlightened days will ever be accepted which does not in its maturity shine on the human race for true knowledge and good.

"All knowledge is our province," said Bacon, and we would be less than men if any phenomenon in nature was considered inscrutable by us, the highest outcome of Nature. Thinking thus, one of the most curious problems is that of the sexes; and the value of the doctrine of Materialism is apparent when we come to question its cause, for no natural law professes even to offer an hypothesis on the subject.

It may here be asked, what is the doctrine of Materialism? As enunciated by the most advanced physicists, it is that "Matter contains within itself the promise and potency of every form and quality of life." This, it will be correctly said, is only a statement, not a *cause*—an assumption that requires proof, not a proposition of fact which may be demonstrated with the facility of a problem in Euclid. Granted; but it will be admitted that if we can show how the sexes originate from matter and its inherent properties, Materialism must be more than an assertion. This without further introduction we now propose to discuss.

Taking matter and its properties as the only foundation we can build on with safety, we ask What is Matter?

After long years of experiment and failure we answer this question with a firm assurance in several things:

*First.* The Indestructibility of Matter. This involves both the eternity of matter and the eternity of the properties of matter. Nothing exists outside of matter. Nothing but matter and its properties exist. Nothing can be taken from matter, nothing can be added to it. Whatever properties matter may have had, matter must have now; and, *vice versa*, whatever properties matter has now, matter has always had.

*Secondly.* Matter is composed of elements of which sixty-four are known. Everything consists of those elements, their combinations, changes, and properties. Whatever form they take now, under similar circumstances they would either in the past or future also assume.

This is the foundation of Materialism, and so far as it goes is perfectly clear and logical. Presuming that no force exists outside of matter, the *properties of matter* must account for every phenomenon in matter, and should they fail the premises fail also, and the fact is made certain that a force exists outside of matter, and *ergo* that Materialism is dead.

What, then, are the properties of Matter?

Here there is confusion and disagreement. Gravitation, cohesion, and chemical attraction are the three forces which have been popularly supposed to control matter; but when Huxley pertinently asked what these forces are, he found them not forces at all, but mere names or effects of a cause or causes unknown. Even Evolution, from which so much was expected and preached, has fallen into disgrace, and proved to be no force or cause either, but merely an "orderly sequence of phenomena" from some cause or causes unknown. How are we, then, to discover those unknown causes? If Materialism be true, they must exist; but Materialism cannot be maintained as a doctrine until we show that they do exist and what they are.

We are thus led back to our premises again—to matter and the elements—and we say, according to materialistic doctrine, if sex exists in matter now, sex must always have existed. Consequently, if matter was once a sheer chaos, or, as the most daring of physicists assert, a universal firemist, then sex in some form or another existed in that chaos or in that mist. As, assuredly, it did not exist in the form of any kind of life we are acquainted with, we are led to ask *if matter does not contain within itself some inherent sexual or dual qualities.* If it does, Materialism is alive; if not, Materialism is dead.

Matter is composed of sixty-four elements, more or less; are these elements all alike in kind, or can we trace a sex or duality in them? Fortunately for our doctrine we can. Although stated by eminent chemists to be of no importance, and made "solely for the sake of simplicity," the elements have long been divided into metallic and non-metallic classes. All the elements belong either

fully studied, and the formation of a community was traced by rearing a simple solitary embryo in an aquarium until a perfect community has been developed from it by budding. During the process of development the law of growth by which the characteristics of the compound organism are brought about was clearly exhibited, and it is fully illustrated by nearly one hundred drawings.

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It may here be asked, what is the doctrine of Materialism? As enunciated by the most advanced physicists, it is that "Matter contains within itself the promise and potency of every form and quality of life." This, it will be correctly said, is only a statement, not a *cause*—an assumption that requires proof, not a proposition of fact which may be demonstrated with the facility of a problem in Euclid. Granted; but it will be admitted that if we can show how the sexes originate from matter and its inherent properties, Materialism must be more than an assertion. This without further introduction we now propose to discuss.

Taking matter and its properties as the only foundation we can build on with safety, we ask What is Matter?

After long years of experiment and failure we answer this question with a firm assurance in several things:

*First.* The Indestructibility of Matter. This involves both the eternity of matter and the eternity of the properties of matter. Nothing exists outside of matter. Nothing but matter and its properties exist. Nothing can be taken from matter, nothing can be added to it. Whatever properties matter may have had, matter must have now; and, *vice versa*, whatever properties matter has now, matter has always had.

*Secondly.* Matter is composed of elements of which sixty-four are known. Everything consists of those elements, their combinations, changes, and properties. Whatever form they take now, under similar circumstances they would either in the past or future also assume.

This is the foundation of Materialism, and so far as it goes is perfectly clear and logical. Presuming that no force exists outside of matter, the *properties of matter* must account for every phenomenon in matter, and should they fail the premises fail also, and the fact is made certain that a force exists outside of matter, and *ergo* that Materialism is dead.

What, then, are the properties of Matter?

Here there is confusion and disagreement. Gravitation, cohesion, and chemical attraction are the three forces which have been popularly supposed to control matter; but when Huxley pertinently asked what these forces are, he found them not forces at all, but mere names or effects of a cause or causes unknown. Even Evolution, from which so much was expected and preached, has fallen into disgrace, and proved to be no force or cause either, but merely an "orderly sequence of phenomena" from some cause or causes unknown. How are we, then, to discover those unknown causes? If Materialism be true, they must exist; but Materialism cannot be maintained as a doctrine until we show that they do exist and what they are.

We are thus led back to our premises again—to matter and the elements—and we say, according to materialistic doctrine, if sex exists in matter now, sex must always have existed. Consequently, if matter was once a sheer chaos, or, as the most daring of physicists assert, a universal firemist, then sex in some form or another existed in that chaos or in that mist. As, assuredly, it did not exist in the form of any kind of life we are acquainted with, we are led to ask *if matter does not contain within itself some inherent sexual or dual qualities.* If it does, Materialism is alive; if not, Materialism is dead.

Matter is composed of sixty-four elements, more or less; are these elements all alike in kind, or can we trace a sex or duality in them? Fortunately for our doctrine we can. Although stated by eminent chemists to be of no importance, and made "solely for the sake of simplicity," the elements have long been divided into metallic and non-metallic classes. All the elements belong either

to one class or the other. So far success seems to favor us. Doubt is the mainspring of progress, and this doubting of a fact which has long been maintained to be of *no importance* may be the key to open up unknown vistas of research.

It will, however, be conceded in a matter of no importportance that this dual classification may be incorrect. This we believe to be the case, for one very important element—hydrogen—is given in every classification among the non-metallic elements, while the element itself is admitted to be metallic; a strange and incomprehensible misplacement. Whether the others are right or not only extensive experiments will determine. With this rectification, however, they are so far correct that the movements of Nature are opened to us as by a miracle. The lock cleared of this obstruction opens readily to the key, and Materialism rules triumphant. We seem premature; how does the duality of the elements solve all mysteries?

The object of this paper was to prove the materialistic origin of the sexes—that sex had its origin in matter. That matter is dual is part confirmation of it, but, like its antitype, we must also prove dual matter to be productive. Two females will not produce, neither will two males. If a production can be formed from the non-metallic elements only, or metallic only, then our theory is false; production should only ensue from the connection or interaction of opposite sexes and elements. Chemical analysis in this particular shows that we are right. *No natural production can be found containing the elements of only one class;* both metallic and non-metallic are essential to a formation. In simple laboratory experiments the opposite elements will combine readily with one another, while combination cannot be produced among the elements of either class alone. Even the simplest natural productions, such as air and water, are of dual combinations. Air composed of oxygen, nitrogen, carbonic acid gas, *hydrogen*, etc. Water, composed of oxygen and *hydrogen*, is the great medium also of life and production. Even the old *element*, fire or combustion, can only be produced from oxygen and *hydrogen*, with other opposing dual elements. All rock formations, crystals, stratas, are produced from combinations of the dual elements. All plants and vegetation are of dual formation and dual in sex, while all animals are undoubtedly male and female.

Our premises being thus clear and true regarding the elements of the matter, it follows that—as all plants and animals are composed of the same elements, of oxygen, hydrogen, etc., in different proportions and combinations—the conclusion we have been seeking is inevitable, namely, *sex in either animal or vegetable life is derived from and had its origin in the duality of matter.*

What causes dual matter to combine and be productive would lead us into another inquiry as to the origin of life from matter; but this we reserve for future consideration.—*Journal of Science, England.*

## THE MECHANICS OF BIRD-FLIGHT.

The mechanics of the flight of birds have been much studied, and considerable space has been recently given to the subject in the columns of the *English Mechanic.* A new contribution has been recently made to a Silesian Society by MM. Legal and Reichel, whose researches deal with the relations of the size of the muscles of flight, and the size and form of a wing-surface, to the power of flight, and a short account may be of interest. (An abstract of the authors' observations appears in a recent number of *Naturforscher.*)

The authors begin by considering the question, whether the absolute size of a bird is of importance with regard to its flying power, *i. e.*, whether two birds, which considerably differ in size from each other, but are geometrically similar in their whole bodily structure, fly equally well. The final answer to this is (as we shall see) a distinct negative. The authors have measured in a great number of birds, the weight of the muscles of flight, especially the most important of these, the great breast muscle, as also its antagonist, the wing raising *musculus subclavius*, and compared it with the body-weight. The ratio of weight of the right and left large breast-muscle to the body-weight varied in the different bird species that were examined, from 1 : 3.4 in the pigeon, to 1 : 10.5 in the gull. But if the bird-species are arranged according to the amount of this quotient, neither the equally good flyers come together, nor birds of equal absolute size; *e. g.*, the partridge stands pretty well forward in the series with 1 : 4.8, near and before the hawk 1 : 5; while the sparrow, stork, and eagle, stand with about 1 : 6 near one another. Certainly, with increasing body-weight, the muscular system concerned in flight does not become relatively greater. The size of the muscles of flight is only one factor in flying-power.

A second, and very important factor is the surface presented by the outspread wing (the wing-surface); and here, again, it is not immaterial in which direction the surface extends. With equal wing-surface, a long narrow surface has more effect than a short and broad one, as a long rudder is more powerful than a short one. The authors have therefore given drawings of the form of the outspread wings for 37 different bird species, and indicated in figures the surface and length (wing configuration). A calculation of the mechanical action showed that where the ratio of the surface and length of the wing to the size of the bird remained the same, the angle of the wing motion and the angular velocity of the wing also remain the same; also that with the absolute size of the bird the air-resistance against the wings increases in the fourth power, but the body-weight only in the third. In order to compare the significance of wing-configuration for flight in large and small birds, one must therefore introduce into the numbers, expressing wing-configuration, a correction according to the absolute size of the bird, and the result of this correction the authors name the wing-number. Now, if the various birds be arranged in series according to wing-number, *i. e.*, according to wing-configuration, with comparative preference of the smaller, the good flyers are found to be at one end of the series, the bad at the other, *e. g.*, partridge 4, wild duck 10, jackdaw 20, sparrow hawk 24, sea-swallow 50. If we now multiply this wing-number with the ratio of the weight of the breast-muscle to the body-weight, *i. e.*, combine the consideration of the actual wing-configuration with that of the relative size of the muscles of flight, which are related to the effectiveness and velocity of wing-beat, we obtain the flight number as measure of the flying power, and this appears, *e. g.*, as follows: Sparrow 0.43, partridge 0.48, wild duck 0.98, jackdaw, 1.72, gull 2.15, kibits 2 92, eagle 2.95, stork 2.97, sea-swallow 3.28.

A comparison of the series thus obtained with the actual flying power, shows that the flight-number in general rises and falls with the flying power and in particular corresponds the better where birds of equal body-size are considered; and less well, the more different the size of the birds compared, so that in larger birds the actual flying power falls behind the comparative flight-number; that even appears, *e. g.*, from a comparison of the partridge with the sparrow. Or conversely, when we compare birds of equal flying power, but different size, *e. g.*, larger and smaller, but adult examples of a species, or species of a genus, the flight-number increases with the body-size. It is indeed difficult and always somewhat erroneous, to measure the actual flying powers of different birds together, one bird accomplishes more in dexterous and quick movements, another in rapid flight in a short time, a third in duration of flight. Still, the result may in general (says the reporter), be regarded as correct. Now, as the flight-numbers express the combined mechanically measurable factors of flight, it follows tha.

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There is a curious fact regarding nickel also worthy of attention. This metal forms solutions, which can be exactly simulated by a mixture of iron and copper solutions; but this mixture contains more iron than that which is complementary to cobalt. Nickel solutions are almost complementary to cobalt solutions; but they transmit an excess of very yellow light. Now, the atomic weight of nickel is nearly the mean of the atomic weight of iron and copper; but it is a little lower, that is, nearer to iron. There is thus a perfect analogy between the atomic weights and the color properties in this case. This analogy is even more general, for Mr. Bayley states that in the case of iron, cobalt, and copper, the mean wave length of the light absorbed is proportional to the atomic weight. The specific chromatic power of the metals varies, being least for copper. The specific chromatic power increases with the affinity of the metal for oxygen. Chromium forms three kinds of salts. Pink salts, identical in color with the cobalt salts; blue salts, identical in color with copper salts; and green salts, complementary to the red salts.

Manganese, in like manner, forms more than one kind of salt. The red salts of manganese are identical in color with the cobalt salts, and with the red chromium salts. The salts of chromium and manganese, according to the author, are with difficulty attainable in a state of chromatic purity. He thinks these properties of the metals lead up to some very interesting considerations.

FIRE AND WATER-PROOF PAPER.—A mixture is made of two-thirds ordinary paper pulp, and one-third asbestos. The whole is then steeped in a solution of common salt and alum, and after being made into paper is coated with an alcoholic solution of shellac.

## DETECTION OF STARCH-SUGAR MECHANICALLY MIXED WITH COMMERCIAL CANE-SUGAR.*

BY P. CASAMAJOR.

In a previous communication on the same subject,† read before the American Chemical Society at the meeting of March, 1880, I gave several processes for the detection of starch-sugar in commercial sugars. One of these consisted in adding to the suspected sugar a quantity of cold water, somewhat less than its own weight, and stirring the mixture for a few seconds. If starch-sugar is present, it will be seen in the shape of white chalky specks.

Quite lately a sample of yellow refined sugar was given to me which was supposed to be adulterated by being mixed with starch glucose. By applying the test just mentioned, there seemed to be left a few small chalky specks, which dissolved after standing a minute or two, making it very uncertain whether any starch glucose was present. Upon repeatedly trying the same test the result was always doubtful.

I was then lead to treat the suspected sugar by a liquid capable of dissolving sugar, but without any solvent action on starch-glucose. After many trials, I found that methylic alcohol of such density as to mark 50° by Gay-Lussac's alcohometer answered the purpose very well, if previously saturated with starch-sugar, as this solution dissolves cane-sugar, either white or yellow, very readily, but does not dissolve starch-glucose.

Methylic alcohol at 50°, saturated with starch-sugar, gives a solution of specific gravity = 1.25. 100 c.c. of methylic alcohol at 50° dissolves 57 grms. of dry starch-sugar, the volume of the solution being 133 c.c. A solution of starch-sugar in ethylic alcohol does not answer so well, because ethylic alcohol does not dissolve so readily the gummy matters found in soft sugars, which are those generally chosen for adulteration with glucose.

To test the presence of starch-sugar in a commercial cane-sugar, the suspected sugar should, in the first place, be thoroughly dried, as otherwise any water present will weaken the alcohol, and enable it to dissolve more starch-sugar. It should then be stirred for about two minutes with the saturated solution of starch-sugar in methylic alcohol. After this, the residue is allowed to settle, and the clear solution poured off. The residue may then be washed with a fresh quantity of the same solution. After stirring again and allowing the residue to settle, there will remain, if any starch-sugar is present, a certain quantity of chalky white specks, accompanied by a fine deposit, formed by the starch-sugar present in power of fine grains. These finer particles are never seen when water is used for detecting the presence of starch-sugar, as they dissolve in water very readily. It seems probable that by using this solution of starch-sugar in weak methylic alcohol, the starch-sugar in an adulterated sample could be estimated quantitatively by a process somewhat analogous to that of Payen for estimating cane-sugar.

Not having had any occasion for such a process I have not determined experimentally the degree of approximation obtainable in this way.

The methylic solution of starch-sugar should be poured on a filter, after it has dissolved all it can from a commercial sugar, and the residue should be washed out with the same solution, and everything poured on a weighed filter. After all the liquid has run off, the filter and the residue may be rapidly washed with the strongest methylic alcohol fuund in commerce, which tests 92½° by Guy-Lussac's alcohometer, and which dissolves starch-sugar with great difficulty.

By a dexterous use of this process it seems probable that very approximate results may be obtained, although what is said here is merely in the nature of a suggestion to those who may have use for quantitative results.

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## COLOR RELATIONS OF METALS.

In a paper on the color relations of copper, nickel, cobalt, iron, manganese, and chromium, lately read before the Chemical Society, Mr. T. Bayley records some remarkable relations between solutions of these metals. It appears that iron, cobalt, and copper form a natural color group, for if solutions of their sulphates are mixed together in the proportions of 20 parts of copper, 7 of iron, and 6 of cobalt, the resulting liquid is free from color, but is gray, and partially opaque. It follows from this that a mixture of any two of these elements is complementary to the third, if the above proportions are maintained. Thus a solution of cobalt (pink) is complementary to a mixture of iron and copper (bluish green); a solution of iron (yellow) to a mixture of copper and cobalt (violet); and a solution of copper (blue) to a mixture of iron and cobalt (red). But, as Mr. Bayley shows, a solution of copper is exactly complementary to the red reflection from copper, and a polished plate of this metal, viewed through a solution of copper salt of a certain thickness, is silver-white. As a further consequence, it follows that a mixture of iron (7 parts) and cobalt (6 parts) is identical in color with a plate of copper. The resemblance is so striking that a silver or platinum vessel covered to the proper depth with such a solution is indistinguishable from copper.

There is a curious fact regarding nickel also worthy of attention. This metal forms solutions, which can be exactly simulated by a mixture of iron and copper solutions; but this mixture contains more iron than that which is complementary to cobalt. Nickel solutions are almost complementary to cobalt solutions; but they transmit an excess of very yellow light. Now, the atomic weight of nickel is nearly the mean of the atomic weight of iron and copper; but it is a little lower, that is, nearer to iron. There is thus a perfect analogy between the atomic weights and the color properties in this case. This analogy is even more general, for Mr. Bayley states that in the case of iron, cobalt, and copper, the mean wave length of the light absorbed is proportional to the atomic weight. The specific chromatic power of the metals varies, being least for copper. The specific chromatic power increases with the affinity of the metal for oxygen. Chromium forms three kinds of salts. Pink salts, identical in color with the cobalt salts; blue salts, identical in color with copper salts; and green salts, complementary to the red salts.

Manganese, in like manner, forms more than one kind of salt. The red salts of manganese are identical in color with the cobalt salts, and with the red chromium salts. The salts of chromium and manganese, according to the author, are with difficulty attainable in a state of chromatic purity. He thinks these properties of the metals lead up to some very interesting considerations.

FIRE AND WATER-PROOF PAPER.—A mixture is made of two-thirds ordinary paper pulp, and one-third asbestos. The whole is then steeped in a solution of common salt and alum, and after being made into paper is coated with an alcoholic solution of shellac.

## DETECTION OF STARCH-SUGAR MECHANICALLY MIXED WITH COMMERCIAL CANE-SUGAR.*

BY P. CASAMAJOR.

In a previous communication on the same subject,† read before the American Chemical Society at the meeting of March, 1880, I gave several processes for the detection of starch-sugar in commercial sugars. One of these consisted in adding to the suspected sugar a quantity of cold water, somewhat less than its own weight, and stirring the mixture for a few seconds. If starch-sugar is present, it will be seen in the shape of white chalky specks.

Quite lately a sample of yellow refined sugar was given to me which was supposed to be adulterated by being mixed with starch glucose. By applying the test just mentioned, there seemed to be left a few small chalky specks, which dissolved after standing a minute or two, making it very uncertain whether any starch glucose was present. Upon repeatedly trying the same test the result was always doubtful.

I was then lead to treat the suspected sugar by a liquid capable of dissolving sugar, but without any solvent action on starch-glucose. After many trials, I found that methylic alcohol of such density as to mark 50° by Gay-Lussac's alcohometer answered the purpose very well, if previously saturated with starch-sugar, as this solution dissolves cane-sugar, either white or yellow, very readily, but does not dissolve starch-glucose.

Methylic alcohol at 50°, saturated with starch-sugar, gives a solution of specific gravity = 1.25. 100 c.c. of methylic alcohol at 50° dissolves 57 grms. of dry starch-sugar, the volume of the solution being 133 c.c. A solution of starch-sugar in ethylic alcohol does not answer so well, because ethylic alcohol does not dissolve so readily the gummy matters found in soft sugars, which are those generally chosen for adulteration with glucose.

To test the presence of starch-sugar in a commercial cane-sugar, the suspected sugar should, in the first place, be thoroughly dried, as otherwise any water present will weaken the alcohol, and enable it to dissolve more starch-sugar. It should then be stirred for about two minutes with the saturated solution of starch-sugar in methylic alcohol. After this, the residue is allowed to settle, and the clear solution poured off. The residue may then be washed with a fresh quantity of the same solution. After stirring again and allowing the residue to settle, there will remain, if any starch-sugar is present, a certain quantity of chalky white specks, accompanied by a fine deposit, formed by the starch-sugar present in power of fine grains. These finer particles are never seen when water is used for detecting the presence of starch-sugar, as they dissolve in water very readily. It seems probable that by using this solution of starch-sugar in weak methylic alcohol, the starch-sugar in an adulterated sample could be estimated quantitatively by a process somewhat analogous to that of Payen for estimating cane-sugar.

Not having had any occasion for such a process I have not determined experimentally the degree of approximation obtainable in this way.

The methylic solution of starch-sugar should be poured on a filter, after it has dissolved all it can from a commercial sugar, and the residue should be washed out with the same solution, and everything poured on a weighed filter. After all the liquid has run off, the filter and the residue may be rapidly washed with the strongest methylic alcohol fuund in commerce, which tests 92½° by Guy-Lussac's alcohometer, and which dissolves starch-sugar with great difficulty.

By a dexterous use of this process it seems probable that very approximate results may be obtained, although what is said here is merely in the nature of a suggestion to those who may have use for quantitative results.

* A paper read before the American Chemical Society, Nov. 4, 1880.

† *Chemical News*, vol. xli., p. 221; *Journal of the American Chemical Society*, vol. ii., p. 111; *Sugar-Cane*, vol. xii., p. 283.

## ASTRONOMY.

### THE LATE PARTIAL ECLIPSE OF THE SUN, AND PENNULE'S COMET.

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In further confirmation I may add that on November 1, $10^h\ 50^m$, Mr. H. Corder, of Chelmsford, observed a bright meteor = Jupiter, which had an apparent path from 275°+56° to 257°+43° and obviously took its departure from the same radiant as that of the fireballs of October 25 and 30.

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The Magnetic observations, embracing the automatically registered curves of the Magnetograph, and observations of Declination, Dip, Deflection and Vibration, seem to indicate the approach of a more disturbed period than has occurred for several years. In order to collect more accurate data relating to this subject, arrangements have been made with other magnetic observatories in different parts of the globe to carry on a series of synchronous observations, and the comparison of the results will probably throw some light upon the laws which govern many of these phenomena. In the Meteorological department, self-recording instruments for the continuous registration, respectively, of atmospheric pressure, humidity, wind (direction and velocity) and rain have been maintained in regular operation throughout the year, in addition to standard eye observations made five times daily for the control of the automatic records. Abstracts of the meteorological results are published weekly.

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WASHINGTON, D. C., *January* 12, 1881.

### THE OBSERVATORIES OF THE UNITED STATES.

I.

CARLETON COLLEGE OBSERVATORY, NORTHFIELD, MINN.

The United States is fortunate in possessing a greater number of well equipped astronomical observatories than any other country in the world. These are distributed over a wide extent of territory, ranging from the shores of the Atlantic to the Pacific coast, and extending from the tropical regions of the Gulf of Mexico, to Lake Superior on the North.

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## ON THE LIMIT OF PLANETARY STABILITY.

By Professor Daniel Kirkwood.

Laplace, in his *Système du Monde*, pointed out the limit at which, according to his estimate, the moon's attraction could have retained an elastic atmosphere.* The question of a satellite's stability was also considered by the late Professor Vaughan, of Cincinnati.† I have seen no attempt, however, to obtain for the different members of our system any definite numerical results. In the present paper it is proposed to find the approximate limits of stability in the cases of the eight major planets and certain of the satellites, on the hypothesis that their primitive condition was either liquid or gaseous.

Let $M$ = the mass of the larger or central body,
$m$ = that of the dependent planet or satellite,
$x$ = the distance from the centre of the former to the limit of stability of the latter,
$a$ = the distance between their centres; then, since the disturbing or separating force of the larger upon the smaller mass is the difference between the attraction of the former on the nearest point of the surface of the latter and that on its centre of gravity, we have

$$\frac{M}{x^2} - \frac{M}{a^2} = \frac{m}{(a-x)^2} \qquad (1)‡$$

or putting $a = 1$ and reducing,

$$x^4 - 2x^3 + \frac{m}{M}x^2 + 2x = 1. \qquad (2)$$

If we adopt the masses and distances given in Newcomb's Popular Astronomy and solve equation (2) for each of the eight principal planets we shall obtain the distance from the centre of each to its limit of stability, as given in the second column of the following table. If, moreover, the planets, with their present masses, be reduced to the sun's mean density their radii as stated in the third column are found by the formula

$$r^n = 430{,}000 \left(\frac{m_n}{M}\right)^{1/3},$$

and the respective ratios of the limits of stability to these radii are seen in column fourth.

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On the assumption that in each case the mean density of the separated mass was equal to that of the central body, the sun's present radius multiplied by the respective numbers in column fourth will give the radii of the solar nebula when the planets extended to their respective limits of stability. These radii are less than the mean distances of the planets in the ratio of 1 to 1.265. This fact may have some significance in regard to the former oblateness of the solar nebula or the law of its density.

*The Earth and the Moon.*—For the moon, which in perigee approaches within 221,500 miles of the earth, the limit of stability is about 38,000 miles. Were the moon's density reduced to that of the earth its radius would be 916 miles, the ratio of which to the limit of stability is 1 : 41.6. The moon's least distance diminished by 38,000 miles is 183,500 miles. If our satellite originally extended to the limit, and if the moon and the earth had the same form and density, the radius of the latter was 165,000 miles.

*The Martian System.*—The diameter of Phobos, according to Prof. Pickering, is 5.57 miles. If its density, therefore, be equal to that of Mars the limit of stability is about two miles exterior to the surface; or, if the density be to that of the primary in the same ratio as the density of the moon to that of the earth, the limit is less than a mile from the surface of the satellite; and finally if the density were no greater than that of water the satellite, if fluid, would be unstable, the limit being actually within the surface. Since, therefore, the satellite could never have existed at its present distance in a nebular state, it must follow, if any form of the nebular hypothesis is to be accepted, that its original distance was much greater than the present. Can we find a probable cause for this ancient disturbance?

If we suppose the former period of Mars to have been very nearly one-sixth that of Jupiter the close commensurability would render the orbit of Mars more and more eccentric. The planet in perihelion would thus pass through the sun's atmosphere, or rather through the outermost equatorial zone of the solar nebula. This resisting medium would not only accelerate the motion of Mars but also in a much greater degree that of his extremely small satellite. The solar mass contracting more rapidly than the orbit of Mars would finally leave the latter moving in an eccentric path without sensible resistance.

*Other Secondary Systems.*—For the first satellite of Jupiter the limit is 5250 miles, or 4½ times the radius of the satellite. For Mimas, the innermost satellite of Saturn, it is less than twice the radius. The rings of Saturn, in all probability, could not exist as three satellites, the limits of stability being interior to the surface.*

The effect of preturbation in the dismemberment of comets is known to all astronomers. The nucleus of the great comet of 1880, which approached within less than 100,000 miles of the sun's surface, must have had a den-

* It has been recently shown that Bessel's mass of the ring is much greater than the true value.

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If we suppose the former period of Mars to have been very nearly one-sixth that of Jupiter the close commensurability would render the orbit of Mars more and more eccentric. The planet in perihelion would thus pass through the sun's atmosphere, or rather through the outermost equatorial zone of the solar nebula. This resisting medium would not only accelerate the motion of Mars but also in a much greater degree that of his extremely small satellite. The solar mass contracting more rapidly than the orbit of Mars would finally leave the latter moving in an eccentric path without sensible resistance.

*Other Secondary Systems.*—For the first satellite of Jupiter the limit is 5250 miles, or 4½ times the radius of the satellite. For Mimas, the innermost satellite of Saturn, it is less than twice the radius. The rings of Saturn, in all probability, could not exist as three satellites, the limits of stability being interior to the surface.*

The effect of preturbation in the dismemberment of comets is known to all astronomers. The nucleus of the great comet of 1880, which approached within less than 100,000 miles of the sun's surface, must have had a den-

* It has been recently shown that Bessel's mass of the ring is much greater than the true value.

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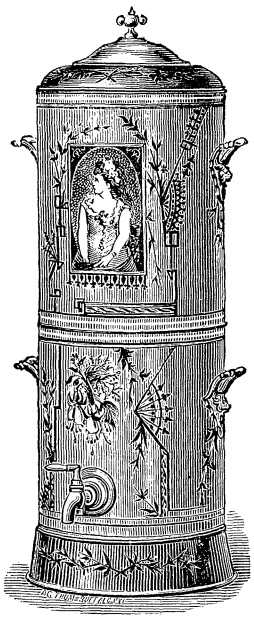

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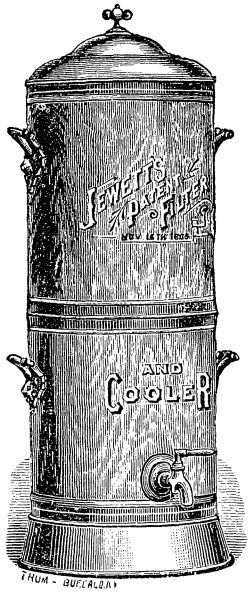


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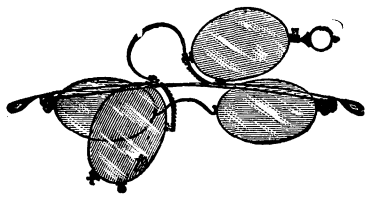

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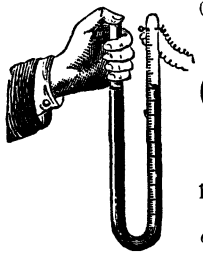

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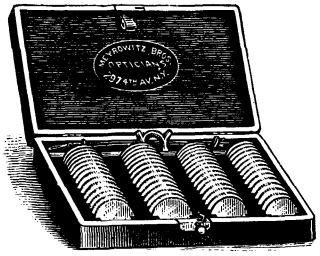

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